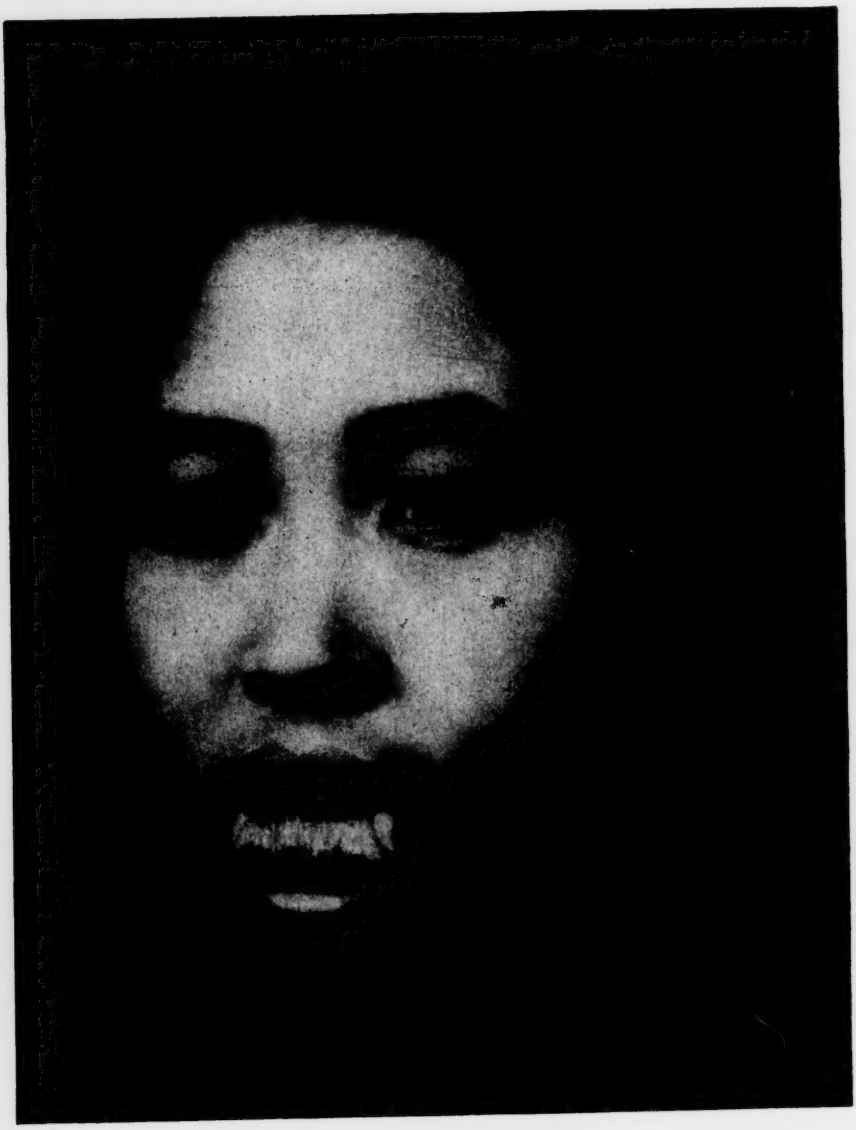
The Journal of HEREDITY



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The

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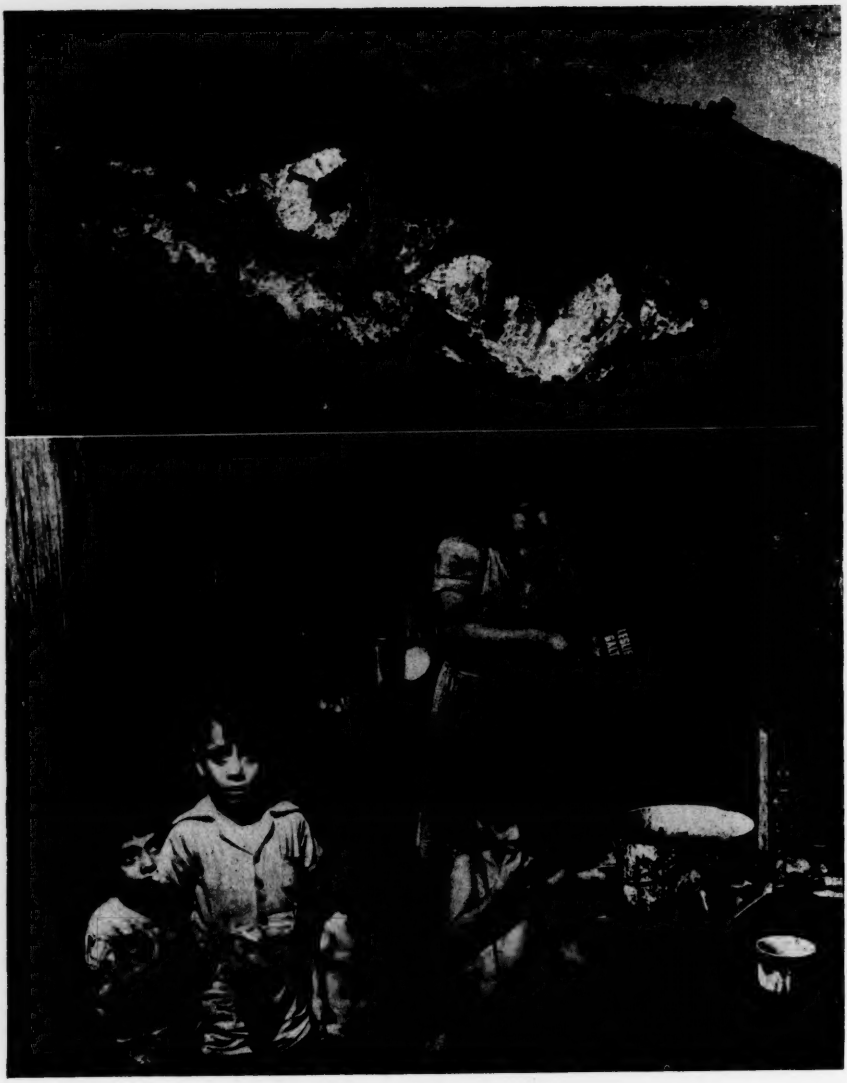


MARCHING SOIL—STARVING PEOPLE

Which Cause: Which Effect?

The upland soils of America are on the march. And in large areas food intake is already below the minimum for adequate nutrition. In El Salvador the per capita intake is less than 1500 calories per day. Says Mexico's Dr. M. E. Bustamante, director general of the Pan American Sanitary commission:

"In Palenque and Salto de Agua we observed the cutting and burning of great areas of forest for the sowing of corn on the mountainsides. This is done without taking steps to avoid erosion, and for this reason, the rains rapidly carry away the topsoil, with the result that the soil is violently impoverished.

"I include this observation because it is definitely tied up with health conditions and the nutrition of the people. In all the farms and villages visited, there is a lack of cultivation of fruit trees, such as oranges, grapefruits and lemons; there are no vegetables, and the diet is based on rice, corn and beans; malnutrition exists, for this reason, throughout the entire zone visited, a fundamental fact in explaining the children's decreased resistance to disease and their physiological poverty."

Upper photograph—near Malipan, El Salvador. The trees are 40-60 feet high. By Marjorie Vogt. Lower photograph: Puerto Rican home, by Delano, Courtesy Puerto Rico Office of Information.

STATIC CONCEPTS IN A CHANGING WORLD

Elementistic Make-Believe and the World We Live In

About ten years ago, a prominent ornithologist and student of ecology began to go wrong. After a number of years of fruitful labor as editor and lecturer for the Audubon Society, William Vogt was appointed in 1939 to the position of consulting ornithologist with the Compania Administradora Guano del Peru. His next two years were spent in a strangely restricted environment in terms of humon contacts, but with no lack of neighbors in the persons of several million cormorants, whose activities, reproductive and eliminatory, created the guano which is a basic resource in the agriculture of Peru.

This experience shifted Mr. Vogt's interest from the intricate mechanisms of bird ecology to the far more complex multiple inter-relations which condition human existence on this earth. Leaving the cormorants to shift for themselves after Pearl Harbor, Mr. Vogt first coordinated Inter-American culture under Mr. Nelson Rockefeller. In 1943 he became Director of the Pan American Union's newly organized Conservation Section. In this capacity he has travelled and surveyed conditions throughout many of the twenty-one American Republics. What he saw in neighboring lands only confirmed what distressed him at home: namely that man seemed to be engaged with almost insane intensity in destroying his environment as rapidly as possible.

Out of this varied experience with birds and people, has come a most remarkable book, Road to Survival. *This world-wide survey of human ecology will be published by William Sloane Associates on August 5, and is Book-of-the-Month Club choice for August. Several hundred thousand readers will be shocked when they learn how destructive we humans have managed to become. Even without the atom bomb our fate seems certain in the light of our achievements within the past century. Critical of the ability of our human species to make a sound ecological go of it in many other parts of the world, Mr. Vogt awards the palm in destructiveness to his fellow citizens in the United States. We of this generation seem determined to better the unenviable record of our forefathers who in three brief centuries plundered a continent. Time to mend our ways is short indeed.*

In the JOURNAL *from time to time, we have discussed the motivating factors which condition group and individual reactions to such basic elements of human ecology as population pressures and control of fecundity. We are privileged to present as a preview of this notable book, the third chapter of* Road to Survival, *wherein Mr. Vogt analyzes how people get that way. It is titled:*

AS MANKIND THINKETH

IN China, the idea that it is necessary to be an ancestor is a cardinal belief, despite the obvious fact that descendants are becoming more wretched with every generation. In starving India, the country is overrun by millions of cattle that cannot be used to satisfy human hunger because they are "sacred"; nor can they be killed and prevented from drawing from the land energy supplies desperately needed for human beings. The *idea* is dominant over the peristalsis of starving stomachs.

When mankind began the massive development of its fore-brain, which is its greatest glory, it also began to lay up trouble for itself on the earth. The shackles of instinct, protectors of the amoeba and the ape, were largely discarded, and Plato became a possibility,

and Moses, Aristotle and Beethoven, and Darwin and Freud. Man was now on his own; he had the freedom to mold his own destiny. Unfortunately, the freedom did not always have its counterpart in wisdom. For this reason, ancient civilizations fell—and our own is tottering.

It has been the fashion, especially of modern years, to emphasize what man has done in the way of material accomplishments. But there has been a strong tendency to neglect what he has thought. For a nation whose founders left the blood of their bare feet in the snow at Valley Forge, we Americans are strangely blind to the power of ideas. We have recently fought our most desperate war, principally because the sick mind of an obscure Austrian so infected an entire nation that its idea of its relationship to Europe and the world was transformed into a virtual paranoia. In Russia we have watched an economic theory develop into a near psychosis that threatens modern nations with the fire and sword of a holy war.

The *idea* of the milpa farmer may be far more difficult to change than is the direction of the gullies through his land. To limit the desire of the Chinese for sons may be a problem more formidable than the provision of at least limited food for them. A slogan may twist the arm of Peace as dangerously as a new weapon. Faith in ancient precepts may be a more effective eroder of soil than two inches of rain in an hour.

Behind nearly every human act lies an "emotion" that sets the act going; and behind the "emotion" lies a "thought" or an "idea." If such survival-emotions as the desire for conservation are to become part of our daily existence, they must be based on knowledge and the thought that stems from it. If we are to make peace with the forces of the earth, that peace must begin in our minds—and we must seek, and accept, many new ideas. We must reject many old ones.

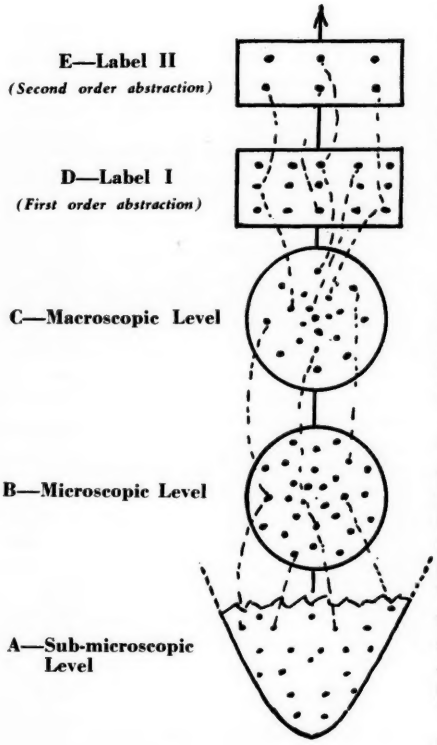
One of the strangest lacunae in human cultural development is the absence of understanding of man's relationship with his physical environment. So anthropocentric has he been that, since he began to achieve what we call civilization, he has assumed that he lives in a sort of vacuum. He has probably been an agriculturist at least a hundred centuries, yet it was only in 1944 that Dr. E. H. Graham published the first book on natural principles of land use.

Ignorance that runs through the entire social structure of the world, from leaders of all stripes to peasants and industrial laborers, is one of the limiting factors that will be most difficult to control. To change agricultural practices in areas of Latin America, Asia and Africa, where the illiteracy rate is often 100 per cent, within a period short enough to save the land will require heroic measures. It is certain that, for all practical purposes, large areas of the earth now occupied by backward populations will have to be written off the credit side of the ledger.

The Dangerous Doctor

The modern medical profession, still framing its ethics on the dubious statements of an ignorant man who lived more than two thousand years ago — ignorant, that is, in terms of the modern world—continues to believe it has a duty to keep alive as many people as possible. In many parts of the world doctors apply their intelligence to one aspect of man's welfare—survival—and deny their moral right to apply it to the problem as a whole. Through medical care and improved sanitation they are responsible for more millions living more years in increasing misery. Their refusal to consider their responsibility in these matters does not seem to them to compromise their intellectual integrity. They have been primarily responsible for making Puerto Rico, for example, one of the most miserable areas on the face of the earth, by expanding the population beyond all possible bounds of decent subsistence, and their present efforts to correct the situation are not much more than tokens. They set the stage for disasters; then, like Pilate, they wash their hands of the consequences.

Nationalism plays an all too effective



THE STRUCTURAL DIFFERENTIAL

Figure 2

The relationship between process level and various levels of abstraction is visualized by Alfred Korzybski in the "Structural Differtial." The "event" is symbolized by a portion of the surface of a parabola, of infinite extent. The higher levels of abstraction pick out various attributes of this infinite-valued "event." Each higher level of abstraction is based on fewer of these attributes, many of which are not capable of expression at verbal levels. See text for discussion. By permission of Alfred Korzybski.

part in reducing the carrying capacity of the world's lands. The attempts at national self-sufficiency, the movement to build local industries in areas that cannot support them, the protection given these industries by tariff barriers, the resultant drag on the free movement of raw materials and goods, force man to exploit his land in increasingly ruinous ways. For a large and rich nation like the United States this nationalism is an effective, if temporary, defense against poverty. How long it can be maintained in the age of atomic and bacterial warfare is dubious. India has already begun to enunciate a Realpolitik that demands expansion.

Unspeakable Reality

Finally, a factor limiting the sound use of natural resources is our Aristotelian heritage, so called because it originated with the great philosopher, and has been developed through the centuries by his followers.

The modern rejection of Aristotelian "logic" is without question as rich in promise of development and clarification for the field of ecology—and its application, conservation—as were the rejections of Euclidean and Newtonian limitations for mathematics and physics. Until we do reject antiquated notions we are likely to remain snarled in the confusion of inferences that are invalid —and the words that express the confusion.

Perhaps the clearest expression of a non-Aristotelian formulation is a graphic device, the "structural differential," originated by Alfred Korzybski, and pictured here in somewhat simplified form. It represents the physical world, and what we think, feel and say about it. Let us relate it, for the sake of our discussion, to an area of land. (It is equally applicable to an apple, a chair, a painting, a blonde, a cathedral, etc.)

This land has an existence at a submicroscopic, colloidal, atomic, etc., level (*A*) much of which is (1948) unknowable. Space and time are not separated in it; modern physics has taught us that we must think of four-dimensional space-time. The land is constantly changing: it is different land June 1, 1948, and June 2, 1948. This is true if we consider only what happens on the submicroscopic level; it is also true, as we shall see later, on the microscopic and macroscopis levels. Therefore we should not think of the land—or a clod of earth in it—as

a static object, but as a dynamic, ever-changing *process*.

By the use of scientific instruments, such as the microscope and pH meter, we can learn many things about the land — structure of the soil, hydrogen-ion concentration, chemical composition, kinds of protozoans present, etc.—as is graphically indicated by the dots in the diagram. What we learn, however, even with the most advanced scientific techniques, cannot be *all* the facts; so the microscopic level (*B*) has less dots than the process level. With our scientific methods we have abstracted certain knowledge but inevitably we left something out. *We cannot know all!* (The "etceteras" so liberally used throughout this book—a cardinal sin against rules laid down by all teachers of English composition I ever encountered—are a recognition of the impossibility of saying *all* about anything. To omit them would be to imply completeness of knowledge and expression, which would be impossible even were unlimited space available.)

As we pass to a still higher level of abstracting (*C*), the macroscopic or objective, where we depend on our unaided senses, our graph must include still fewer dots, showing that still more is left out. At each step we are further removed from *all* the "truth," even as it is currently understood. At this level we cannot possibly understand colloidal structure, chemical composition, functioning of organic matter, molds and their influence on plant life, etc. Still more is omitted; still greater is our ignorance. This is the nearest many of us will approach, in our abstracting, to the *process* that is the land. This is the level at which live most farmers and gardeners, some congressmen and teachers, and others who have firsthand knowledge. It is because we live at this level that we maintain soil surveys, agricultural experiment stations, agricultural colleges, etc. The people who work in them have a wider knowledge on the microscopic level; in their abstracting, they omit less.

These three levels might be called the levels of reality. They are limited to things that—someday, if not yet—may be weighed, or measured, or felt, or seen. But they may not be *said*. They exist at what Korzybski calls the "unspeakable level." When we begin to talk about them we necessarily use a higher order abstraction—leave out still more. And it is here that much of our trouble begins.

The first verbal level of abstraction is the label (*D*), the name, or description —"land." This does not smell sweet after a rain, or wash into our rivers, or raise fifty bushels of corn an acre, or stick to our boots. "The word is *not* the thing"—but many of us act as though it were. For businessmen, such as many real estate agents; for millions of asphalt-bound city dwellers; for many economists and national and international officials, there is no differentiation between the *word* land and the *process* or *object* land. The two are identified in thought and speech. This partially explains why thousands of unfortunate refugees are being dumped into tropical countries that do not have nonverbal land capable of feeding their own people.

(If this discussion of abstracting is not immediately understandable, it may be helpful to think of the structural differential in terms of the blonde. In one's abstractions, how much is necessarily omitted, and how many problems have complicated the lives of how many men because of their assumption of "allness" —that they know *all* about her! How many young men, scarcely aware of the "unspeakable" blonde, have fallen head-over-heels in love with the *verbal* level, including its luscious inferences, and identified it with the *object* level! Pin-up girls, wolf-calls, the popularity of certain movie stars—and of Reno—all bear witness to these identifications of levels of abstraction, the possible danger of "allness.")

Higher levels of abstraction are possible almost without limit—and they offer even greater opportunities for trouble. Let us look at a word like "forest" as applied to land. This is a still higher abstraction from level *A*. Miles of forest

have been destroyed to make tons of paper on which to verbalize about "forest" land. But what is meant by it? The Woolgrowers' Association says it *is* range. The forester, even at this late date, is likely to say it *is* a source of timber. The wildlife manager says it *is* game habitat. The hydrologist says it *is* watershed protection. The Mexican politician says it *is* corn land. Et cetera. The word "forest," in this case, indicates an *inference* about the land, and each of these people abstracts something different — draws a different inference. Few of them speak with a consciousness of abstracting or a realization that much is omitted. In this way difficulties arise that often lead to tragic results, wasting billions of dollars, years of men's lives, and millions of acres of good land.

The lack of consciousness of abstraction, the assumption that one knows all, and says all, in a given situation, dams the minds of leaders and followers alike, leads to oversimplification, and "solves" extremely complex problems with many factors unconsciously omitted. This sort of thinking is especially characteristic of legislative bodies and world organizations. It leads to the writing and acceptance of documents like the Communist Manifesto and the Atlantic Charter. It tricks man into seeking political and/or economic solutions for problems that are political, economic, social, geographic, psychological, genetic, physiological, etc.; it is like the old application of the clyster as a cure-all.

The Brake of Confusion

The confusion of the levels of abstraction leads us to identify what happens on the purely verbal level with the process on the subverbal level. For example, a woman squeezes the breakfast oranges, sips the juice, and thinks, "How sweet they are." Her husband rushes out a few moments later, takes a few mouthfuls of flapjacks and maple sirup, and then drinks his orange juice. "Gosh, these are sour oranges!" he exclaims.

They are, for all practical purposes, the same oranges. It is the husband—including maple sirup—that is different. Or, more exactly, what has happened at the subverbal level—the relationship of the juice to taste buds, affected in one case by sirup, but not in the other—is the process; "sweet" and "sour" are merely words. Instead of considering the total situation as mapped by the structural differential, the husband has looked at only one isolated level—*one element.*

This leads us to consideration of what Korzybski has called "elementalistic thinking." The various individuals, in their reactions to forest land, mentioned above, have thought in elementalistic terms. This type of reasoning has been enormously harmful in our fumbling attempts to deal with the problems of human survival and decent living standards. It constantly bewitches sociologists into trying to settle "population" problems apart from the total environments in which populations live. It is partly responsible for the antics of such people as Army Engineers, who seem literally incapable of understanding the necessity of beginning flood-control work on the hilltops. It straps many of our foresters into the strait jacket of timber production, and isolates even the concept of multiple use of forest lands from the exceedingly pertinent implications of economics, sociology, hydrology, and national and international affairs. It holds the agriculturist, as he is often proud to state, "down to earth," and separates his thinking—and especially his education—from the undeniably related concerns of logic, economics, semantics, education, languages, broad land-use organization, history, anthropology, etc. It keeps many economists wandering in a symbolical cosmos situated somewhere between heaven and hell, as was the world of the thirteenth-century theologian. It leads international organizations to seek solutions in Economic and Social Councils that, by definition, omit from their calculations such indispensables as geography, climate, psychology, carrying capacities, folkways, and population developments, all of which are, themselves, dynamically interrelated. It is responsi-

ble for the self-satisfaction of the medical man, smug in his conviction that his chief responsibility is to postpone death as long as possible, under whatever circumstance.

The fact that most of us live mainly on the verbal level leads to verbal splitting that has no justification at the level of reality. We talk and think of "body" and "mind"—though modern physicians have begun to recognize their unity in psychosomatic medicine. "Space" and "time," "emotions" and intellect" are separated in words, and we accept the separation as though it were characteristic of reality. (This discussion, separating "thought" from emotion and action, is defensible—in non-Aristotelian terms—merely as a matter of convenience. In reality, of course, thought, action, and emotion are more inextricably intertwined than Laocoön and his serpents.)

The Aristotelian, two-valued thinking has plagued even the most advanced technicians in fields related to the management of natural resources and problems of human populations. The terms "good" and "bad" are used as absolutes, without recognition that there is a gradation between them and that every good includes much bad, and vice versa. For example, the "good" hawk and the "bad" hawk have kept game commissions bemused, and Audubon societies excited, for decades. "Cause" and "effect" are used as explanatory terms without the realization that every "cause" is in itself an "effect," and every "effect" a "cause." Conservationists are prone to insist that they are "men of action" as opposed to "theorists," or "men of thought," despite the fact that conservation activity, divorced from sound thinking, has enormously wasted our funds and our resources. "Conservation" has been so long sterilized by isolation from "education"—when they are, in reality, inseparable—that many Ph.D.'s are ignoramuses in questions having to do with the land; and a shocking proportion of our State Conservation Commissions are guided by traditions that should have disappeared with the Model T.

Identification — what Korzybski calls "is-ness"—results in the failure to recognize that all phenomena, from individual cornfields to individual countries, are *unique* and (as processes) *constantly changing*. But the problems of Great Britain and those of Iran are analyzed on an identical basis. The same medicine, perhaps labeled Democracy, is prescribed for Albania and for the United States. Young men are brought from all the world for training and indoctrination under the TVA, on the assumption that TVA's should be prescribed for their own particular brand of national or regional rickets. Soil conservation methods, of proved soundness in the United States, are transplanted bodily to Latin America, where they are a failure because of wide disparities in geographic and cultural conditions. Economists who will change their car every two years, their ideas on nutrition and child raising every ten, cling to nineteenth-century concepts such as the fallacy of Malthusianism, as though they were divine revelation.

Finally, another source of recurrent confusion is our failure to recognize what Korzybski calls "multi-ordinal terms." These are terms indicating abstractions in varying degrees, terms that are meaningless apart from the context in which they are found. Such notions as "security" and "standards of living" are multi-ordinal terms. A high standard of living for the son of a successful insurance salesman in Chicago — that is the reasonable satisfaction of his desires —is a very different thing from the high standard of living of a pottery maker in Tlaquepaque. A "national park" is not the same phenomenon in Venezuela as it is in Arizona. "Freedom from want" in Timbuctoo has little relationship to "freedom from want" in Dubuque.

Identity, elementalism, "allness," etc. —direct descendants of Aristotle's logic — have so shaped our thinking and, therefore, our treatment of our land and associated resources that they often exert as powerful an influence as rainfall and soil structure. As the basis of sound resource management, the necessity of

recognizing and rejecting these mistaken concepts can scarcely be overemphasized.

Two decades ago, the oldest man in a little Hudson Valley town married the village idiot, many years his junior. His explanation was that he hated to get into a cold bed alone.

Most of us are in a comparable situation. Our concepts, evolved twenty centuries ago, may be idiotic in an overpeopled, atomic age, with much of the world a shambles. Yet we cling to them because of an instinctive fear of lack of support if we give them up.

They were magnificent concepts in their day; their survival through all these centuries is clear proof of that. But they can no longer serve us, except as millstones about our necks. We live in a world of change, a world of infinitely complex relationships. As relationships change, so do values. They are relative and (in the mathematical sense) functional. Static values, any kind of static thinking, are as obsolete as the Roman chariot.

Modern science has taught us that, in the realms of mechanics, of medicine, of physics, etc. We must also learn it in our relationships with the earth and each other—if we are to survive.

GRANTS AND FELLOWSHIPS IN CANCER RESEARCH

THE Committee on Growth of the National Research Council, Acting for the American Cancer Society, is entertaining applications for grants and fellowships. Applications for extension of existing Grants in Cancer Research will be received until 1 October; applications for new grants until 1 November. Final decision on applications submitted during this period will be made in most cases soon after 1 February. Grants approved at this time ordinarily will become effective 1 July 1949.

Fellowship applications may be submitted at any time. Those received prior to 1 November will be acted upon by the Committee on Growth in December. Those received between 1 November and 1 March will be acted upon in April. Fellowships ordinarily will begin 1 July though this date may be varied at the request of the applicant.

During the past year the American Cancer Society, Inc., on recommendation of the Committee on Growth has approved research grants and fellowships totaling over $2,000,000.

Communications regarding grants and fellowships should be addressed to Executive Secretary, Committee on Growth, National Research Council, 2101 Constitution Avenue, N. W., Washington 25, D. C.

THE "EYEBROW" GENE
in Nemesia strumosa

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MODIFICATIONS of the usual F_2 ratio have been known since shortly after the rediscovery of Mendel's laws. In one of these the dominant allele of one pair obscures the effect of the alleles of a second pair and the F_2 ratio becomes 12 : 3 : 1. The dominant genes whose effect is the same no matter which of the other alleles is present is said to be *epistatic* and the alleles of the other pair which result in a different character only in the absence of the dominant gene of the first pair are said to be *hypostatic*. This terminology was suggested by Bateson[1] who termed the relationship *epistasis*. It has been termed *dominant epistasis* by Snyder[5] to distinguish it from the situation in which a recessive gene obscures the effect of the alleles of another locus, thus producing a 9 : 3 : 4 ratio.

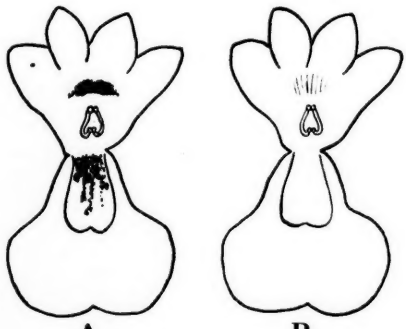
Most of the epistatic genes that have been reported affect the same area of the plant or animal that is affected by the corresponding hypostatic genes. For example, East and Hayes[2] reported that two genes affecting the aleurone color in maize show this relationship. The offspring from a selfing of a *Prpr Rr CC* plant segregate into 9 purple : 3 red : 4 white, and in each type the same areas of the grain are affected. Sinnott and Durham[4] reported a similar situation in squash; two pairs of genes result in a ratio of 12 white : 3 yellow : 1 green and, again, all three types involve the same area of the fruit. Numerous other examples could be listed.

The "Eyebrow" Character

In *Nemesia strumosa* the writer has found a pair of alleles that are hypostatic to the basic anthocyanin gene *C* and also to the flecked type but which do not exhibit their effects on the same areas affected by *C* or by the genes producing flecked. They are the gene for "eyebrow" and its allele.

In *cc* or in non-flecked plants the eyebrow character appears as a small, dark purple blotch on the inner surface of the upper lip of the corolla near the tube (Figure 3*A*). Accompanying this are a number of very small dots and patches of dark purple on the inside of the lower part of the tube. The "eyebrow" on the upper lip is fairly similar in size and shape in different plants, but the markings on the lower part of the tube vary from just a few dots to large blotches that cover most of the tube. In most flowers the color is so dark as to appear black. In non-eyebrow plants the dark eyebrow is replaced by a few thin lines varying in number in different plants and the dark spots on the lower part of the tube are absent (Figure 3*B*). Apparently the two types are influenced by modifying genes for there was considerable variation in some families and some of the plants were hard to classify. These families are not included in this discussion.

A few simple tests indicate that the "black" pigmentation in the eyebrow and tube might be the result of anthocyanins. When exposed to ammonia vapor these regions turn blue or greenish. In hydrochloric acid vapor the eyebrow appears to be unchanged in some plants but to turn reddish in others and when the flower is dipped in dilute hydrochloric acid the eyebrow turns pinkish slowly. The spots on the tube also become red in hydrochloric acid vapor. In sulphur dioxide the eyebrow becomes paler; the dark spots in the tube turn somewhat reddish, become very pale, and sometimes fade out almost entirely.



EYEBROW AND NON-EYEBROW
Figure 3

A—the eyebrow type on a white or yellow flower. *B*—the non-eyebrow type on a white or yellow flower. All *C* plants have the eyebrow character regardless of their "eyebrow" genotypes.

Inheritance of Eyebrow

Eyebrow appears to be dominant over non-eyebrow. In white or orange types lacking gene *C* and the flecked character, four crosses between eyebrow types resulted in 492 eyebrow and 0 non-eyebrow. One cross between two non-eyebrow plants gave 0 eyebrow and 21 non-eyebrow. Three families from a cross between an eyebrow and a non-eyebrow consisted of 128 eyebrow plants only. Eleven F_2 families segregated into 331 eyebrow and 112 non-eyebrow which is very close to a 3 : 1 ratio. The symbols *E* and *e* will be used for the eyebrow and non-eyebrow genes respectively.

When *C* is present this segregation is not obtained, for *all* plants with the basic red or purple anthocyanin pigmentation have the dark eyebrow on the upper lip and the dark spots on the lower part of the tube (Figure 3*A*). Gene *C*, then, appears to be epistatic to the *E* and *e* alleles. Unfortunately, no crosses were made to demonstrate the 12 : 3 : 1 ratio, but one family from the cross *cc Ee* × *Cc Ee* consisted of 16 colored, 11 non-colored eyebrow, and 3 non-colored non-eyebrow. Four families from the backcross *Cc Ee* × *cc ee* consisted of 44 *C* (*E*), 24 *c E*, and 18 *c e*.

If *C* is epistatic, some colored plants which are phenotypically eyebrow should be genotypically *ee*. This was tested in three ways. Three families from crosses between a colored type and one that was *cc ee* consisted of 49 colored and 55 non-colored non-eyebrow with no *c E* plants. Eight families from a cross between a colored and a *cc Ee* plant segregated into 200 colored : 95 *c E* : 93 *c e*. The colored parents of these eleven families must have been *Cc ee*. Three families arose from crosses in which both parents were of this genotype. Two purple-flowered, phenotypically eyebrow plants when crossed gave 25 purple and 10 white non-eyebrow. Reciprocal crosses between two red-flowered plants gave respectively 26 red and 6 orange non-eyebrow and 22 red and 8 orange non-eyebrow.

All the above crosses indicate the epistasis of *C*. The most interesting feature of this relationship is that the epistatic gene results in anthocyanin over the inner part of the lower lip and much of the upper lip whereas the dominant hypostatic gene produces a small patch of color on a part of the upper lip not covered by the anthocyanin pigmentation from the epistatic gene and small spots on the tube, a region apparently unaffected by the basic color gene. This relationship is very different from that of eyebrow in combination with the blue and blue-margin types (Riley, unpublished).

Linkage Relations

There is no evidence of linkage between *E* and the self-sterility alleles. In a number of families in which one *e* allele was common to the two parents the ratio of *E* to *e* was too close to 3 : 1 to suggest any gametic elimination. The ratios given previously indicate no linkage between *C* and *E*. Several families suggest that *E* is not linked with *O*. Two families from the cross *oo ee* ×

Oo Ee segregated into 9 *O E* : 17 *O e* : 11 *o E* : 12 *o e*, and four families from the cross *Oo Ee* × *Oo ee* segregated into 75 *E* : 106 *O e* : 24 *o E* : 29 *o e*. One family indicates that there is no linkage between *E* and the recessive gene for yellow. The cross *Yy Ee* × *yy Ee* segregated into 41 *Y E* : 13 *Y e* : 29 *y E* : 6 *y e*. There are no data to indicate whether *E* is linked with the genes for glass green and yellow spot. Some evidence indicates no linkage between *E* and the gene for buff.

There is some evidence for linkage between *E* and pale upper. In a cross between a *pp ee* plant and a double heterozygote the offspring were 7 *P e* : 0 *P e* : 0 *p E* : 7 *p e*, indicating that the heterozygous parent might have been *P E/p e*. In a cross that might have been *P e/p e* × *P e/p E* the ratio was 4 *P E* : 11 *P e* : 4 *p E* : 1 *p e* against expected ratios of 7.5 : 7.5 : 2.5 : 2.5 for no linkage and 5 : 10 : 5 : 0 for complete linkage. On the other hand, one similar cross did not fit the expectation based on linkage.

Flecked

An inherited type not yet discussed is flecked. In flecked plants numerous irregular dots or short lines of purple or red anthocyanin appear on the lower lip of the corolla. This type will be described elsewhere, but it seems appropriate to mention here that the flecked type also appears to be epistatic to eyebrow since all flecked types that have been found are phenotypically eyebrow even in families in which the non-flecked types are segregating for eyebrow.

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Puerto Rico's Record Increase

Statistics from the Puerto Rico Monthly Statistical Report, April 1948, show that for the seven-year period 1941-1947, inclusive, the average annual birth rate was 41.2, the death rate 14.8, and the increase rate, births over deaths, was 26.4. This rate, if continued, would cause the population to double in about 27 years.

But these summary figures do not tell the whole story. Since 1941 the birth rate of Puerto Rico has increased from 39.8 to 43.4, and the death rate decreased from 18.6 to 12.0.

In 1946 Puerto Rico had an increase rate, births over deaths of 29.5, and the provisional figures for 1947 showed an increase rate of 31.4. These increase rates of 29.5 and 31.4 are probably the highest recorded in all human history. If continued they would cause a doubling of the population in less than 25 years.

Before Puerto Rico became a possession of the United States at the turn of the century, it had high birth and death rates, and a slow increase of population. The island was already overcrowded as is indicated by the fact that it had 280 people per square mile in 1900 as compared with only 26 people per square mile in the United States. However, in 1946 Puerto Rico's density of population had increased to 618 per square mile compared to 47 in the United States.

The population of Latin America is now about the same size as that of the United States. But, as already has been mentioned, Latin America seems to be increasing at a rate that might double its population in about 35 years. Now if the death rates of Latin America are lowered, as the death rates of Puerto Rico have been, and there is no significant lowering of the birth rates, we can expect the large areas of Mexico, Central and South America to become dangerously overpopulated in the near future.—*Population Bulletin.*

MODIFICATION OF X-RAY EFFECTS

On Barley Seeds by Pre- and Post-Treatment With Heat*

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THIS is a report of a preliminary investigation to determine to what extent exposure to heat, either before or after X-raying, alters the biological effects of the irradiation. Our data were derived from two series of treated seeds of barley which were exposed to various combinations of irradiation and heat treatments, as described below.

Methods of Treatment

Dormant seeds of Himalaya barley were used in the tests. The heat treatment was set at a temperature and duration just below the lethal point as determined (only a day or two before the experiments were run) from samples of the same stock of seed as that used in the tests. Seeds were placed on a piece of cardboard and exposed to dry heat in an electric oven. In the second series it was necessary to lower the temperature to 65°C. in order not to kill the seeds, possibly because of a higher moisture content of the seeds at the time the test was run, which is known to lower heat tolerance. Unfiltered radiation was applied at approximately 1,000 *r* per minute, at 15 milliamps and 26KVP.

In the two tests made, the experimental control populations comprised the following:

Test No. 1

(1) 3,000 seeds were given 15,000 *r* units;
(2) 870 seeds were subjected to a temperature of 75°C. for 30 minutes;
(3) 3,000 seeds were given 15,000 *r* units and then immediately subjected to a temperature of 75°C. for 30 minutes;
(4) 3,000 seeds were subjected to a temperature of 75°C. for 30 minutes and immediately exposed to 15,000 *r* units.

Test No. 2

In Test 2 the same categories were used as in Test 1, but only 100 seeds were given each of the treatments. The X-ray dosage was increased by 5,000 *r* units to 20,000 *r* units, and the heat treatment was decreased from 75°C. to 65°C. for 30 minutes.

Fifty of the treated seeds from each lot in both tests were germinated in petri dishes, and root tips from them were fixed in Carnoy's solution. In Test 1 the remaining seeds were planted directly in the field, while in Test 2 they were planted in greenhouse flats. A considerable number of the root tips were examined for chromosomal bridges at late anaphase and early telophase using the acetocarmine smear technique. The frequency of such bridges is considered to be a reliable measure of the frequency of induced chromosomal aberrations.

Observations

In the root-tip analyses of both tests, heat alone produced few if any bridges, but heat either before or after irradiation reduced the frequency of bridges resulting from irradiation by more than half. The combined data from both experiments are presented in Table I.

Notes on survival, height, and average number of culms per plant were taken at near maturity on the plants in the field, and these data are presented in Table II.

There was no pronounced difference among the three irradiated lots in other characteristics, but there was a strong indication that heating the seeds after irradiation increased their ability to survive as compared with the survival of seeds heated before irradiation. However, data at present being collated (using dormant and germinating seeds of both barley and einkorn, but not includ-

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ed in Table II) seem to indicate that injury symptoms (and survival) vary with dosage, particularly of X-rays. That is, in some tests, injury from heat has been alleviated by irradiation, and in other tests pre- or post-treatment with heat has alleviated the injury resulting from X-rays.

Table III presents data on frequency of seedling mutants in progenies of the plants in Table II. The classification of "mutants" and determination of "mutation rate" followed the method of Stadler.[14] These data indicate that pre- and post-treatment with heat may increase the mutation rate of X-rayed seeds. There was a significant increase in the mutation rate in seeds given X-radiation followed by heat treatment as compared with the rate in seeds given irradiation only. However, since these data are based on only one test, it is preferred not to emphasize this difference.

Discussion

It has been shown by a number of investigators* that heat (or cold) modifies the biological effects of X-radiation. The exact process, whether it be physical, chemical, mechanical, or a combination of any two or all three, is a matter receiving intensive study at the present time. In some studies conclusions have been based on evidence from cytologic or cytogenetic observations, while in others they have been based on macroscopic observations on the degree of injury or frequency of death of the treated organism. Whether there is a necessary correlation between the frequency of cytogenetic aberrations and injury or killing is a question. It has been suggested by Frőier and Gustafsson,[2] and assumed or stated by others, that a positive correlation does exist between the frequency of chromosomal aberrations and the survival and vigor of treated organisms. Such a correlation was doubted by Kempton and Maxwell[5] and was not observed by Smith[13] in a study of an "X-ray susceptible" mutant in einkorn. If there were a necessary correlation between X-ray injury and chromosomal aberrations, a big difference would be expected in the frequencies of bridges in root tips from seeds of barley given certain combinations of treatments, because there are marked differences in degrees of injury. There was no clear difference in frequencies of bridges, but there were clear differences in injury in the combined, heat plus X-ray and X-ray plus heat, treatments from which the data for Tables I and II were obtained. It is doubtful that reduction in moisture content was a major factor in the modification of X-ray effects herein reported. This conclusion is based on the observations that heat applied before or after irradiation to either dormant or germinating seeds (insofar as data have been obtained) apparently has the same effect on injury symptoms and chromosomal aberrations resulting from X-radiation.

Sax and Enzmann[11] suggested that the production of breaks in chromosomes by X-rays is independent of temperature. They felt that the effect of high temperature during irradiation may be due to an acceleration of the fusion of broken ends, increasing the proportion returned to

TABLE I. Frequencies of bridges after X-ray, heat, and combination treatments of dormant seeds of barley (summary of two tests). In Test 1 15,000 r-units of X-radiation and a heat treatment of 75°C. for 30 minutes were used. In Test 2 20,000 r-units and 65°C. for 30 minutes heat were used.

Treatment	Seeds germinated	Cells examined	Bridges observed	Bridges per cell
X-rays	100	222	146	0.66 ± 0.03
Heat	100	75	5	0.07 ± 0.03
X-rays before heat	100	194	59	0.30 ± 0.03
Heat before X-rays	100	260	66	0.25 ± 0.03

TABLE II. Effect of X-rays, heat, and combination treatments on survival and growth of dormant seeds of barley. X-ray treatment, 15,000 r-units; heat treatment, 75°C. for 30 minutes.

Treatment	Treated seeds planted No.	Plants obtained No.	Survival %	σ	Estimated ave. height Inches	No. culms Ave.
X-rays	2,950	859	29.1 ± 0.83		12	8
Heat	820	499	60.8 ± 1.70		15	9
X-rays plus heat	2,950	918	31.1 ± 0.85		13	7
Heat plus X-rays	2,950	580	19.6 ± 0.73		13	8

*Herskowitz,[3] Kaufmann *et al.*,[4] King,[6] Maxwell *et al.*,[7] Mickey,[8] Sax and Enzmann,[11] Smith,[12] and a number of others.

original positions, which could account for the observed effect of the heat. The implication from this supposition is that heat facilitates fusion of broken chromosomes for only such time as the chromosomes are actually in the heated condition. If this is correct, then heat applied before or after X-radiation would be expected to alter aberration frequency for such reasons as these: (1) Heat applied prior to irradiation might be retained by the seeds for some part of the time during which irradiation is taking place. (2) The broken chromosomes retain their ability to fuse for a time after the breaks occur, and heat applied during this interval would aid in fusion of the broken ends. Sax[10] showed that induced breaks usually undergo restitution, or form new associations, within a relatively short time after they occur. He observed, however, that a few breaks may "remain open for perhaps hours." Sax reasoned that if heat aids in restitution and recombination of broken chromosomes, then heat treatment immediately after irradiation should be most effective. His tests did not substantiate this hypothesis because temperatures imposed after irradiation had no effect on the frequency of aberrations. Neither do the data in Table I, where it is apparent that there was little difference between the frequencies of bridges in root tips from seeds heated either before or after irradiation.

Kaufmann *et al.*[4] found that pre-treatment of X-rayed male Drosophila with infra-red radiation increased the frequency of detectable rearrangements. Post-treatment with infra-red immediately after X-raying did not seem to be effective in altering the frequency of breaks. They suggest that the effect of infra-red on the observed frequency of chromosomal rearrangements induced by X-radiation could be due to an increase in temperature. Kaufmann's data are at variance with the data presented in Table I, where it is evident that heat reduced the frequency of bridges resulting from irradiation. This could conceivably be due to one of the following causes: (1) The effect of infra-red on the frequency of chromosomal rearrangements is not a heat effect; or (2) there is a fundamental difference in the response of dormant barley seeds and of Drosophila to combined heat and X-ray treatments. Possibly the respiratory system of animals, even of flies, makes it more difficult to increase the temperature of their protoplasm than it is to increase the temperature of the protoplasm of dormant seeds, and besides animals will not survive the high temperatures that dormant seeds will tolerate.

In studies of X-ray-induced fragmentation of chromosomes in microspores of Tradescantia,[1,9,11] it was found that increasing the temperature during irradiation lessened the frequency of aberrations. However, Sax and Enzmann[11] reported that "the temperature effect is reversed when the chromosomes are irradiated at earliest prophase, when the chromosome is split to form sister chromatids," as compared with the temperature effect when X-ray induced chromosomal aberrations were produced at mid-prophase. This suggests that the state of the chromosomes in relation to metabolism and cell division has a marked bearing on the biological effects of combination heat and X-ray treatments.

The data presented in Table III indicate that pre- and post-treatment with heat may increase the mutation rate of X-rayed seeds. When Tables I and III are compared, it is apparent that the seeds subjected only to X-rays had the highest frequency of bridges, but the lowest frequency of mutations. This relationship is not in agreement with the usually accepted concept that a correlation exists between frequencies of X-ray-induced chromosomal aberrations and mutation rate. These observations are being further checked.

TABLE III. Effect of X-rays, heat, and combination treatments on dormant seeds of barley on mutation rate. X-ray dosage, 15,000 *r*-units; heat treatment, 75°C. for 30 minutes.

Treatment	Head progenies No.	Mutants No.	Heads with mutants % σ
X-rays	1,591	92	5.78 ± 0.6
Heat	641	2	0.31 ± 0.2
X-rays before heat	2,363	216	9.14 ± 0.6
Heat before X-rays	2,103	145	6.89 ± 0.6

Summary

1. The frequency of chromatinic bridges in root-tip cells of barley was reduced by half when dormant seeds were subjected to a barely sub-lethal heat treatment either immediately before or immediately after X-radiation.

2. Heat treatment after irradiation seemed to increase the mutation rate in X-rayed barley seeds as compared with the rates in seeds treated with X-rays alone or with heat before irradiation.

3. There was by no means a perfect correlation between the frequency of chromatinic bridges, mutation rate, or injury resulting from different treatments or combinations of treatments.

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"All the Rice in China" Not Enough!

Population increases in South and East Asia continue to out-run increases in the production of rice—the "staff of life" for nearly half the people of the world—the Food and Agriculture Organization of the United Nations reports in Rice Bulletin No. 1, just issued by the Organization.

The lag in production is not a recent development, FAO notes. World production of rice for decades has failed to keep pace with the growth of the rice-eating population. In the period between the two wars production of rice in South and East Africa increased by less than 10 per cent, while the population increase in the region amounted to more than 20 per cent. The rate of increase of the basically rice-eating population of the world approximates 10 millions each year. To feed these additional people, world production of rice needs to be increased annually by at least 1.3 million metric tons (milled or cleaned basis).

World War II brought about a sharp reduction in production in South and East Asia, which has normally been by far the most important rice surplus area of the world and which has now become a huge deficit area. Expanded production in the Western Hemisphere and in Africa (an outstanding development of the war and postwar periods) has only partly compensated for the decline in Asiatic production.

Although world-wide rice production in 1947-48 was back almost to the pre-war level, the amount available for export during the calendar year 1948 is less than 40 per cent of the quantity of rice that moved in international trade annually during the five years 1934-38. The smaller availability for export reflects, first, the fact that the large exporting countries in Asia have not yet recovered their prewar levels of production, and, second, that they, in common with other territories in Asia, have greatly increased populations to feed.

The acute shortage of rice in South and East Asia, where more than 90 per cent of the world's rice is consumed, has also created an abnormal demand for substitute food grains, which are themselves scarce. It has also induced greater consumption of oilseeds in Asiatic producing areas and so has reduced the supplies available for export to Europe and America. Thus the rice shortage has had a serious effect on the food economy of the world as a whole.

The meagerness of typical ration scales in the deficit areas is shown by the following examples of basic rations in effect during December 1947:

	Rice Ration Per Person Per Day		*Caloric Equivalent*
	Oz.	*Grams*	
Malayan Federation	4.6	130.4	457
Ceylon	4.6	130.4	457
Singapore	4.8	136.1	477
Sarawak	6.4	181.4	636
Hongkong	7.5	212.6	745
India	4.3	121.9	427

—*F.A.O. News Release.*

THE HISTORY OF HYBRID CORN

Critical Comments on Richey's Review of Crabb's Book

A REVIEW of a book is ordinarily so much a matter of the reviewer's opinion that to take issue with it is merely to pit one reader's personal opinion against another's, an inherently futile procedure. But when the review is clearly guilty of the very offenses of which the book itself is accused, including "gross misplacement of emphasis" and "false implications," then it becomes time to examine the review.

I do not offer a blanket defense of Crabb's book *The Hybrid-Corn Makers,*[1] reviewed by Richey[5] in a recent number of this JOURNAL. Some of the criticisms which Richey makes of the book are valid by any standards. His criticism of the indefensible overemphasis on Holbert's work will receive virtually unanimous approval. But to damn the book as a product of the seed and advertising trade and to impugn the author's motives (while disclaiming any intention of impugning the author's motives) by injecting such quotations as "bought like a bullock, hoof and hide" is both unfortunate and unfair. As one who has been associated during periods of his life with both the seed trade and government bureaucracy, I can testify that the one is not completely venal nor the other wholly virtuous.

Despite its obvious faults (some of which the author must himself by now have recognized) the impartial reader will find much of interest and value in Crabb's book. It contains a wealth of new material bearing upon the history of hybrid corn, much of it obtained by the prodigious labors of the author through personal interviews and correspondence. Some of this material might soon have been permanently lost except for these labors. The early history of hybrid corn as recorded in Chapters II to VII is in my opinion the most accurate history of this period which has so far been written. It reveals evidence of a perspective which has been conspicuously lacking in some of the previous accounts of hybrid corn, particularly those written by persons who have themselves participated in its development, or by writers who are not aware that history is something more than a compilation of the published record with the principal emphasis on priority of publication. I have no doubt that some impartial historian in the future, freed from the profit motive on the one hand and from the equally powerful urge for personal prestige on the other, will find a welcome harvest of historical material in Crabb's book and will be grateful to him for it.

I take issue with Richey most sharply in his treatment of the contributions of Donald F. Jones, and I do this not in his defense since he is quite capable of defending himself, but because my version of the history of hybrid corn is in many respects at complete variance with Richey's. Richey is of the opinion that Crabb's book "grossly exaggerates the influence of Dr. D. F. Jones in the development of hybrid corn." He is entitled to that opinion. He is not in logic or fairness entitled to support that opinion with the kind of evidence which he brings to bear upon it, nor to publish it as the objective judgment of an impartial observer. Richey's statement on Jones' contributions falls into three categories: 1) belittling the importance of the double cross; 2) challenging the originality and significance of Jones' theory of heterosis; 3) denying the importance of Jones' influence on other corn breeders.

In my opinion, Richey's comments on the double cross result in a distorted picture of its importance. True, the double cross is not a profound advance in scientific theory, and no one, so far as I am aware, has ever claimed that it is. It is an invention and fundamentally a simple invention, as have been many other important inventions, including some of the great inventions which have marked turning points in man's history. At the

same time, however, the double cross is a brilliant and highly effective, and by no means purely accidental, solution to the most important problem in hybrid corn breeding, the problem of seed production. It is an invention which marked an important turning point in the history of hybrid corn, for it made the difference almost at once between hybrid corn as a theoretical possibility and hybrid corn as a practical reality. Hence, to speak of it in context with other disparaging remarks as "only a mechanical modification" of previous methods is comparable to speaking of the wheel, one of the greatest of all of man's inventions, as "only a mechanical modification of a log rolling down a hill." Richey's statement is not false but it surely represents a choice of words calculated to belittle an invention which in a few short years changed the entire history of corn improvement.

A number of faults are found with Jones' theory of dominant linked factors to account for heterosis. In one instance this theory is dismissed as not original since the concept of favorable dominant factors was earlier formulated by Bruce and by Keeble and Pellew. Next, East is cited as stating that the essence of Jones' theory is not dominance but linkage. It is then pointed out that the general concept of linkage was established not by Jones but by T. H. Morgan and others (a fact known to many college freshmen and never to my knowledge disputed). Next, Collins is cited as showing that the concept of linkage is unnecessary in any case. If the theory had been of no historical importance before, it has now become so through the very multiplicity of the faults which Richey has found with it. And in this process of fault-finding the historical significance of the theory has been almost completely missed. It *did* represent a synthesis of earlier theories with modern genetic concepts. It *did* set geneticists to thinking of heterosis in Mendelian terms to an extent which early theories had not, and it *did* stimulate new interest and experimentation on the phenomenon of heterosis. I wonder whether Richey's own experiments on convergent improvement[4] are not an outgrowth of Jones' theory rather than of the theories of Bruce or of Keeble and Pellew.

Finally, Richey denies that Jones had any great part in recruiting the hybrid corn breeders of the country. Perhaps the argument here hinges on the definition of the word "recruit." There is no doubt in my mind that he exercised a marked *influence* upon the corn breeders of the period immediately following World War I. His papers and the book, *Inbreeding and Outbreeding,* published jointly with E. M. East, were widely read and received world-wide approbation. He carried on a voluminous correspondence with corn breeders and experiment station workers in various parts of the United States. He visited and gave advice to workers throughout the Corn Belt and in return he received a constant stream of visitors at New Haven, including such distinguished corn breeders as Henry Wallace and F. D. Richey. Whether these men were "recruited" or "influenced" by Jones or by his publications is a question which only each can answer individually in the honesty of his own mind and memory. I know it to be a fact that in at least one of the cooperative Federal-State programs which Richey later planned and carried so effectively to a successful conclusion, the spade work was done by D. F. Jones.

What Happened in 1918?

It is possible, furthermore, to demonstrate from unbiased data that *something* of importance must have occurred in about the year 1918 to create a new interest in hybrid corn. The data for this demonstration are found in an appendix to an article on corn improvement which appeared in the 1936 *Yearbook* of the United States Department of Agriculture.[2] Here in Table 2 are listed all the stations, state and federal, at which inbreeding programs were then in progress and the years in which they began. In the decade following the publication of Shull's first paper, inbreeding programs

were started at five stations, and at none, so far as I know, were inbred strains being utilized in the practical production of hybrid corn. Even Shull had given up his experiments on inbreeding and crossing corn.* In the decade from 1918 to 1927 thirty-two stations, state and federal, initiated programs of inbreeding corn. Richey can scarcely claim credit for all of these since many (10) were initiated before he assumed charge of the corn breeding program in the United States Department of Agriculture in 1922, and many others (14) initiated subsequently were not in cooperation with the United States Department of Agriculture. This same article states that Henry Wallace who had been inbreeding corn since 1916 did not begin a systematic program of inbreeding until 1919.

Why this sudden and almost nation-wide passion for inbreeding corn? Had Shull's and East's earlier papers, like that of Gregor Mendel, been suddenly and dramatically rediscovered? Or was Jones' invention of the double cross possibly a factor? Or his explanation of heterosis? Or his published papers and his book with East in which were presented adequate and convincing experimental data on the effects of inbreeding and crossbreeding? I leave the answers to the impartial historian.

I doubt that a completely accurate and authentic history of hybrid corn will be written during the lifetime of those who have played important roles in it. On the other hand, the historian of the future will undoubtedly welcome the views and opinions of those who have had an opportunity to see history in the making. As one who has had that opportunity I give it as my considered opinion that there has been a growing tendency on the part of a small but influential group of corn breeders to minimize the importance of the contributions of D. F. Jones in the history of hybrid corn. I point to Richey's review of Crabb's book as the most recent example. But this is by no means the first. As the first conspicuous published example of this tendency I cite the article on corn improvement mentioned above in the 1936 *Yearbook* of the United States Department of Agriculture, published while Richey was Chief of the Bureau of Plant Industry and hence in a position to approve or disapprove its contents. Here in an article of forty pages Jones' contributions to corn improvement are disposed of in exactly three short sentences; less space than is devoted to Cotton Mather's observations on cross fertilization in corn. Is this a fair picture? Is this impartial reporting in a medium intended to give the public accurate and impartial information?

I prefer to believe that this peculiar emphasis (or lack of emphasis) has been innocent rather than intentional. In either case it has had the unfortunate effect of also minimizing the importance of the contributions of East and Hayes,† for the work of Jones is only the logical culmination of the sustained and long-range program of experimentation on the effects of inbreeding and crossbreeding conducted at the Connecticut Agricultural Experiment Station since 1905, first by East, later by Hayes and finally by Jones.

Accent on Continuity

To me the most important single factor in the history of hybrid corn is that even in this early period of its history there was at one of our agricultural re-

*In one of his recent papers Shull[16] states that he discontinued his experiments on corn because he regarded his part in the development of hybrid corn as having been completed. In the light of letters written by Shull in 1914 (quoted by Crabb,[1] p. 66) in which he stated that he never regarded his method as practical, this recent statement has the earmarks of rationalization.

†A conspicuous example of this is found in a recent article by Morrison[3] in which East's role has been reduced to that of an obstructionist.

search institutions such a sustained and continuous program of basic research. Without it Jones could not have made the contributions which he made. Without it Shull's researches, abandoned before they reached practical fruition, might still be meaningless so far as improving the world's food supply is concerned. Without it we should certainly not have had hybrid corn as soon as we did, indeed we might not have it even now. Viewed in this light, the history of hybrid corn is much more than a matter of giving proper priority and due credit to the personalities involved. It is a story of sustained cooperative effort, of continuity in research, of emphasis upon principles at the expense of immediate profits.

As such, the history of hybrid corn is but another example emphasizing the vast importance of sustained programs of basic research whether in agriculture, industry, or national defense. Any historical interpretation which tends to obscure that fact is, in my opinion, not only narrow, but also foolish and in this period of the world's history, perhaps even dangerous.

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MR. RICHEY REPLIES

Dr. Mangelsdorf has courteously supplied me with a copy of his comments on my review of *The Hybrid-Corn Makers,* with the suggestion that I might wish to reply in the same number.

I had been told that Crabb worked for the agency which handles Funk's advertising. I had no way of knowing whether the book as a whole was written as advertising and paid for as such or not. If it was, I do not think that "bought like a bullock, hoof and hide," to which Dr. Mangelsdorf takes exception, would be extreme language. Since I did not know, I so stated and added that it was immaterial whether this was the case or whether the distortions came about from other causes.

My review did not damn the seed trade, but stated that "most members of that trade have followed the highest ethical standards." The shortcomings were emphasized because they were omitted completely from Crabb's book, —which was the subject being reviewed.

For the rest, Dr. Mangelsdorf's criticisms are concerned primarily with what he considers my unfair evaluation of Jones' contributions. First, he accuses me of deprecating the importance of the double cross, originated by Jones. My statement that "the use of double crosses undoubtedly advanced the general use of hybrid corn by many years," hardly suggests an intention of belittling its importance. But I did object to what I considered the undue emphasis achieved for it by minimizing other contributions *in the book under review* and so stated.

Dr. Mangelsdorf's second point is my challenging the originality and significance of Jones' theory of heterosis. As Dr. Mangelsdorf correctly states that theory, it refers to "dominant linked factors." I fail to find on pp. 88-89 of Crabb's book, however, any reference to the "linkage" part in the theory — the only part that was original with Jones. I also find no reference to Bruce's earlier, elegant contribution, or to Collins' paper showing that the concept of linkage is not necessary to the acceptance of the original explanation. Undoubtedly, Jones' interpretation of heterosis in terms of linked genes brought about an earlier

(*Continued on page* 185)

GENETICS IN COCKER SPANIELS

Observations on Heredity and on Physiology of Reproduction in American Cocker Spaniels

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SOME time ago when this laboratory began a long-time program of genetic studies on dogs, a plan to enlist voluntary cooperation by dog breeders was adopted. This plan includes free blanks mailed out by the laboratory and filled in by the individual breeder. The first blank is mailed out by the American Kennel Club without charge. Subsequent blanks are provided on request. By far the largest group of records, within one breed, thus far received from fanciers throughout the country is that of American Cocker Spaniels.

The courteous and helpful cooperation of the American Kennel Club in distributing blanks for initial contacts, and of the individual breeders in filling out and forwarding these blanks has made it possible for us to present at this time the results of a study on the first 337 litters recorded.

Discussion of the Material

All but a few of the blanks submitted were completely and carefully filled out. Occasionally a breeder would record the sex of the pups in a litter but not their color or he would omit the color of one of the parents or fail to note the "order" of the litter in relation to the breeding record of the dam. In such cases the data have been included only in those tabulations where definite information has been elicited. For this reason the total number of litters or of puppies may not be the same in all tabulations.

In recording coat color there is so much room for individual interpretation of different shades of red pigment that all dogs showing any degree of red or yellow pigment with *no* black have been grouped together as "red." This does not mean that all such dogs are necessarily produced by the same combination of genes conditioning color. It *does*, however, give a group which can be contrasted with those having black pigment only or with those having a "black and tan" pattern.

Coat Color

1. Black and red

When "black," "red" and "black and tan" are considered without reference to whether white also occurs in the coat, the following results are obtained:

Mating	Progeny		
	Black	Red	Black & Tan
Black × Black	556	73	9
Black ♀ × Red ♂	220	165	1
Red ♀ × Black ♂	231	131	14
Total Black × Red	451	296	15
Red × Red	19	445	0
Total	1026	817	24

These results are in agreement with the findings of previous investigations by Barrows and Phillips[1] and by Phillips.[3] They support the hypothesis that extension of black pigment over the coat is dominant over restriction of dark pigment to produce red coat color in its many shades.

The relationship of red to "bicolor," i.e. black and tan, is not at all clear and needs further study with careful observation of litters from matings planned to yield critical information.

There appear to be at least two genetically distinct types of restricted (i.e. red or yellow) coat colors in dogs. One of these probably lies between full extension of dark pigmentation (solid colored black or liver) on the one hand, and the pattern known as "bicolor" producing black and tan or liver and tan. The three types of coat which we call "extended," "tan" and "black and tan" respectively are probably conditioned by a series of triple alleles designated E, e^t and e^b.

Another type of red or yellow conditioned by the R-r alleles, is independent

of the "tan" series. Whereas in crosses with e^t individuals the *r* allele reconstructs full pigmentation such as black or liver, it is hypostatic to e^b (black and tan) instead of being epistatic to it as is e^t. Reds of this type are characterized by a recessive form of "restriction" which we may call *rr*. Non-red dogs with respect to this locus are *RR* or *Rr* in formula.

Experiments with both types of "red" are being carried out and many breeds still remain to be classified. At present, however, it seems that the following tentative groupings can be made:

red (yellow) of the **"e^t" type** (genotype e^te^t *RR* or e^te^b *RR*)	red (yellow) of the **"*r*" type** (Genotype *EErr* or *Ee*^{t}*rr*)
Basenjis	English Setters
Dachshunds	Poodles
Chows	Dalmations (red type)
Irish Terriers	Irish Setters
Brussels Griffons	Cocker Spaniels (?)
Collies (sables)	
Shetland Sheep Dogs (sables)	
Chihuahuas	

It may happen that further breeding tests will lead to the formation of one or more other groups or to exchange of breeds between the two groups described. It may also be possible for *both* kinds of "red" to occur independently in the same breed and such a situation cannot be excluded until carefully controlled breeding tests have been conducted at a scientific laboratory.

2. Spotting

In Cocker Spaniels there are two very different types of spotting. One of these produces the varieties known as particolor (if red, liver or black alone is present with white) or tricolor (if large white spots occur on dogs with the black and tan or liver and tan pattern). In all such animals there are chiefly large blotches or patches of white or colored coat. When white occurs on the muzzle, legs or feet there may be flecks or "ticking" of color in those areas.

The other type of spotting has small white areas only on the muzzle, feet or belly. It closely resembles a type of spotting in rats known as the "Irish" pattern and for purposes of brevity will be so designated in this paper.

Animals with the latter type of spots are not desirable in Cockers. The spots are essentially "faults" in the eyes of the bench show judge. They keep cropping up, however, even in the descendants of solid colored dogs and are fairly common in pedigrees involving crosses of solid colored dogs with parti-colors. This latter fact suggests that there may be "Irish" patterned dogs which occur as heterozygotes between solid and particolored individuals as well as "Irish" patterned dogs resulting from a genetic change in the solid colored gene. The results of tabulating the various types of mating which involve spotting are as follows:

Matings	Solid (S)	Irish (s^i)	Particolor (s^p)
Solid × solid	1074	247	27
Solid × Irish	101	67	11
Solid × Parti	66	47	33
Irish × Irish	2	17	5
Irish × Parti	1	11	7
Parti × Parti	2	4	91

Although Phillips and Barrows quoted by Whitney[4] state that "spotting is dominant," Whitney himself shows on page 167 a particolored Cocker with the statement "black and white is recessive to solid color." Wriedt[5] suggests that solid animals heterozygous for particolor may show what we have called the Irish pattern. Haldane[2] favors a series of triple alleles S, s^i and s^p.

There is no evidence against his hypothesis to be derived from the figures just given. On the other hand, there is a great deal of variation in the amount of spotting in both "Irish" and particolor varieties. It would seem probable that, in addition to three chief centers of variation as expressed by S, s^i and s^p there are multiple modifying factors which may influence the degree to which the main genes can express themselves.

The clearest evidence that "solid" is epistatic to both "Irish" and "Parti" is derived from the crosses solid × parti which gives 66 solid to 33 parti and between solid and Irish which gives 101 solid to 67 Irish. The ratio of 2 solid : 1 parti closely parallels the ratio of 1.5

dominant blacks : 1 recessive red obtained in the crosses between those two colors.

Fertility and Sex Ratios

An unexpected difference between sex ratios occurs when progeny of black mothers are compared with progeny of red mothers. Whether this difference will be confirmed when larger numbers are available, and what it actually means is uncertain at this time. The actual figures obtained were as follows:

Mating	Progeny		Sex ratio
	Males	Females	Males per 100 females
Black ♀ × Black ♂	340	296	114.8 ± 3.9
Black ♀ × Red ♂	208	187	111.2 ± 5.0
Black ♀ × any ♂	548	483	113.4 ± 3.4
Red ♀ × Red ♂	258	259	99.5 ± 4.4
Red ♀ × Black ♂	190	191	99.4 ± 5.1
Red ♀ × any ♂	448	450	99.5 ± 3.1

The difference between the sex ratio of the total progeny from black mothers and that of the total progeny of red mothers is 3.2 times its standard error and is probably significant. It provides one of those interesting cases of apparent maternal influence on the sex ratio which could most easily be explained by conditions inside of the female reproductive tract resulting in selective activity or differential survival rate of the two types of sperms.

There is another method of grouping data that brings out an interesting difference in sex ratio. This is the ratio of "first" litters as compared with "subsequent" litters. The sex ratios of progeny derived from these two groups are as follows:

	♂	♀	Ratio of ♂s per 100 ♀s
1st litters	484	426	113.6 ± 3.5
Subsequent litters	509	502	101.3 ± 3.6
Difference	12.3 ± 5.0		2.4 × SE*

*Since tabulating the above, the sex ratios of 115 more litters have been calculated. The combined total now shows a difference of 13.4 ± 4.1 or 3.26 times its standard error. This is probably significant.

Taken by itself this difference is of doubtful significance but considered in the light of similar comparisons in other forms the results are interesting.

Differences of the same type resulting in an excess of males have been recorded for first births in humans and also for first litters of mice as compared with subsequent litters.

It would seem, therefore, that it may very well be a phenomenon generally displayed among mammals.

The incidence of stillbirths and of various types of abnormality is also of interest. Taking first the stillbirths and including among them puppies which died within the first 48 hours as representing a group which was probably non-viable from the start the following tabulation has been made:

Litter order	Total pups	Stillborn or non-viable	% born dead
1st	910	52	5.7
2nd	492	22	4.4
3rd	323	11	3.4
4th	98	2	2.0
5th	70	2	2.8
6th	16	3	18.7
7th	18	1	5.5*

*Numbers probably too small to have any great significance as regards percentage of incidence. There is a steady decrease in incidence in litters until after the fourth or fifth when adverse intra-uterine conditions evidently begin to be manifested.

The sex ratio of those born dead or non-viable is as follows:

	Males	Females	Ratio of males to 100 females
1st litter	27	21	128.5
Subsequent litters	25	16	156.2
Total	52	37	140.5 ± 12.7

This sex ratio differs from that of the total births by 2.6 times its standard error. This approaches significance although the numbers in this group are not large enough to make these deviations statistically conclusive. The excess of males is not significantly different in first litters from that of the normal young in those litters. In subsequent litters, however, the differences are of greater magnitude.

This is in agreement with the fact that "subsequent" matings show by their relative "selection" against males that this process might be expected to continue to operate at the threshold of birth and immediately thereafter.

Other abnormalities recorded in these matings are as follows:

	♂	♀	Total
Overshot jaw	2	2	4
Undershot jaw	4	5	9
Harelip	3	3	6
Cleft palate	1	2	3
Eye "tumor" lid	1	2	3
Deformed leg	2	0	2
Deformed nipples	0	2	2
Runt (dwarf?)	1	1	2
Skull not grown together	0	1	1
	14	18	32

It is interesting to note that no apparent excess of males occurs in the incidence of these abnormalities. Their number is too low for statistical analysis.

Size of Litters

A tabulation of the distribution of litters by size of litters and by order of litter is given in Table I. From these data the average number of young per litter for successive litters can be calculated. It is as follows:

Litter number	Average young per litter
1st	5.55
2nd	5.50
3rd	5.95
4th	4.71
5th	5.46
6th	6.00
7th	4.50

Ordinarily one would expect the size of second litters to be slightly greater than that of first litters. Whether the present data represent a chance deviation or an actual exception to that rule is not clear. Larger numbers will be needed to determine this point.

The *distribution* of litter size according to birth is the reflection of another physiological process of interest. Among mice, first litters are more variable in size than second litters, and third litters are the least variable of any. Variability of litter size again increases after the third. This is shown by an examination of the standard deviations of litters recorded in Table I, according to their sequence:

Litters	No. of litters	Standard deviation
1st	160	2.0
2nd	85	2.3
3rd	56	1.7
4th-7th	42	2.4

Here again the variability of second litters represents an exception to the situation usually reported. The significance of this must be considered as doubtful until further data are obtained. The first, third and subsequent litters present the usual relationship found in comparable material.

One other matter of possible interest remains to be considered. This involves the question of the existence of a possible seasonal influence on the sex ratio. Whitney has tabulated a number of litters of different breeds of dogs raised under relatively standard conditions of environment. He grouped together litters born during the six-months period of December through May inclusive and formed another group of those born from June through November.

In the first group he recorded 299 males and 215 females. In the second group, 499 males and 434 females. These are ratios of 139.0 ± 6.0 and 114.9 ± 3.5. The difference between them is 24.1 ± 6.5 or 3.4 times its standard error. This seems to be a significant difference.

Our own figures for Cocker Spaniels raised under different environmental conditions are as follows. We have used the same monthly groups as those of Whitney.

	♂	♀	Ratio
December-May	583	549	106.2 ± 3.0
June-November	416	392	106.1 ± 3.6

The two ratios are essentially the same. There is no evidence of seasonal differences in the sex ratio.

Tabulating the combined data of Whitney and our own for several breeds raised under different conditions the same two periods give the following totals:

	♂	♀	Ratio
December-May	882	764	115.4 ± 2.7
June-November	915	826	110.8 ± 2.4

The difference is 4.6 ± 3.6 or 1.27 times its standard error. It appears that the results reported by breeders as a whole do not support the existence of a seasonal difference in sex ratio. Whitney's own data still represent a challenging subject for further study to determine whether a constant difference exists under the environmental conditions his dogs were exposed to. If the difference is consistently maintained over a period of years, it will be important to analyze the factors which produce it.

TABLE I. Number of Young Per Litter by Order of Litters

Order of litter	1	2	3	4	5	6	7	8	9	10	11	12	13	14	Total
First	1	7	9	20	37	42	18	16	6	3	1	0	0	0	160
Second	0	4	7	7	18	27	10	5	5	2	0	0	0	0	85
Third	0	0	6	4	11	13	12	8	2	0	0	0	0	0	56
Fourth	3	0	3	8	0	2	3	1	0	0	0	0	0	1	21
Fifth	1	0	1	1	2	5	1	2	0	0	0	0	0	0	13
Sixth	0	0	0	1	0	2	0	1	0	0	0	0	0	0	4
Seventh	0	0	1	1	1	1	0	0	0	0	0	0	0	0	4
Total	5	11	27	42	69	92	44	33	13	5	1	0	0	1	343

Summary

To sum up the results, it may be said that the data derived from cooperators have given an opportunity to utilize numbers far larger than those which would otherwise be available for study.

In spite of minor omissions from certain of the data, it has been possible to obtain valuable information on color inheritance, stillbirths and various other abnormalities, litter size and sex ratios under different conditions.

The data are presented without as complete bibliographic references as would be desirable because of the destruction of the Laboratory's library by fire at the time that this paper was being written.

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✦

History of Hybrid Corn

(*Continued from page* 180)

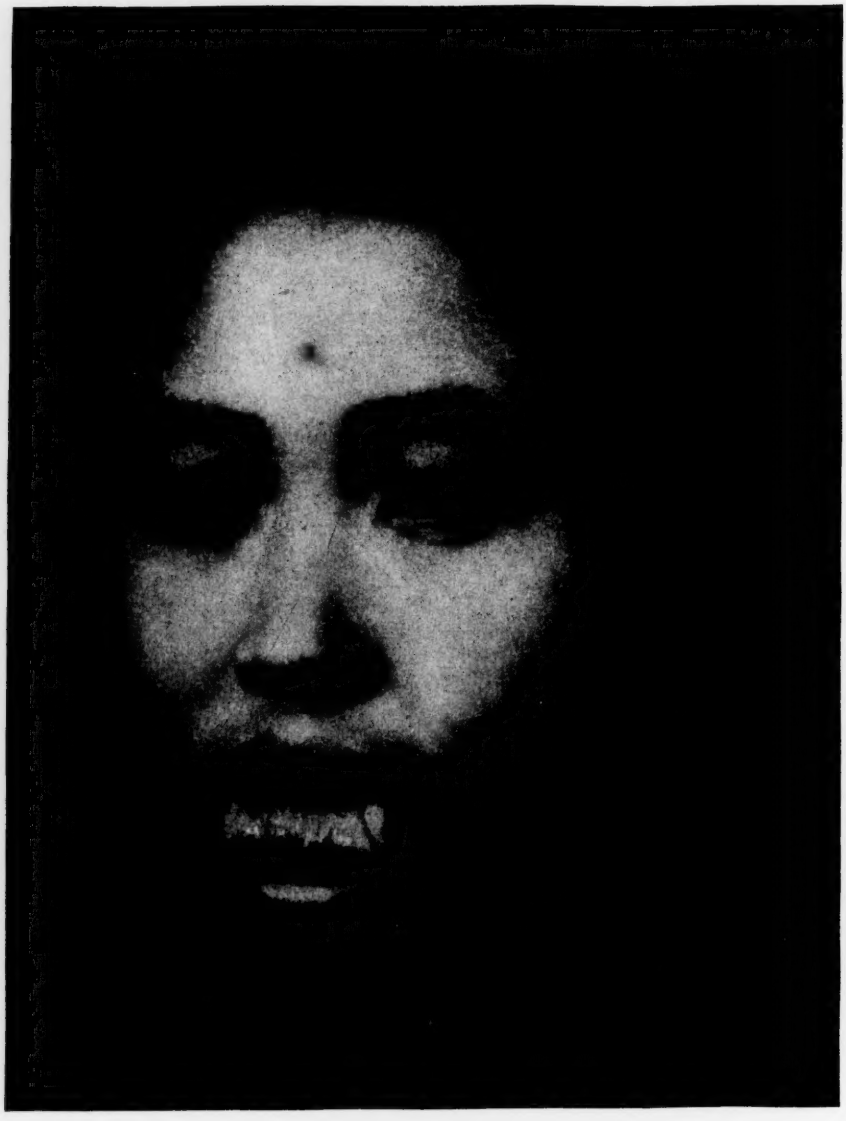
acceptance of the dominant favorable interaction element. But I still think Crabb's book gives a biased story of this development.

Dr. Mangelsdorf's third criticism is that I denied the importance of Jones' influence on other corn breeders. I stated, as my considered opinion, that the book under review grossly exaggerated that importance. I also noted that this must be a question of opinion, and it is pertinent to emphasize that such opinion involves two elements: I agree that Jones exerted much influence, but I still think Crabb's book exaggerated that influence, specifically in connection with "recruiting the hybrid-corn makers of the corn belt."

Dr. Mangelsdorf's statement that "Richey can scarcely claim credit for all these" [32 programs of inbreeding]; his "considered opinion" that there is a small but influential group of corn breeders who are minimizing the importance of Jones' contributions; and his linking Richey with that group, require little comment—they speak for themselves. It should be noted, however, that *if* there is a "small but influential group" of corn breeders taking a uniform point of view, that point of view could well arise by parallel logical processes from a basis of common intimate knowledge of the facts. As I pointed out in my review, Crabb's book records little or nothing of the extensive and successful research in hybrid corn breeding methods accomplished since the early 1920's. This resulted largely from the cooperative efforts of the State-Federal corn breeders of the Corn Belt. Possibly there are others who feel as I do that those advances were one important factor in giving hybrid corn to American agriculture, and also that the contributions of Shull, of Bruce, and of Keeble and Pellew should not be by-passed entirely or damned with faint praise. If so, they may think that what to Dr. Mangelsdorf seems "minimizing" the importance of Jones' contributions, in reality is but an attempt to maintain a proper perspective. Such a point of view could be reasonably sound, and less biased, perhaps, than that of Dr. Jones himself or than that of Dr. Mangelsdorf who was Dr. Jones' assistant from 1921 to 1927.

It seems unnecessary to add more. Those who are interested will have read my critique, Dr. Mangelsdorf's comments on it, and this reply. I will be content with their judgment.

Frederick D. Richey



UP-FOLDED TONGUE

Figure 4

Ability to curl the tongue (see Figure 6) is not a very rare characteristic. In China members of certain families have been found who are "folders" rather than curlers. This ability appears to be hereditary.

TONGUE UPFOLDING

A Newly Reported Heritable Character in Man

T. C. Hsu

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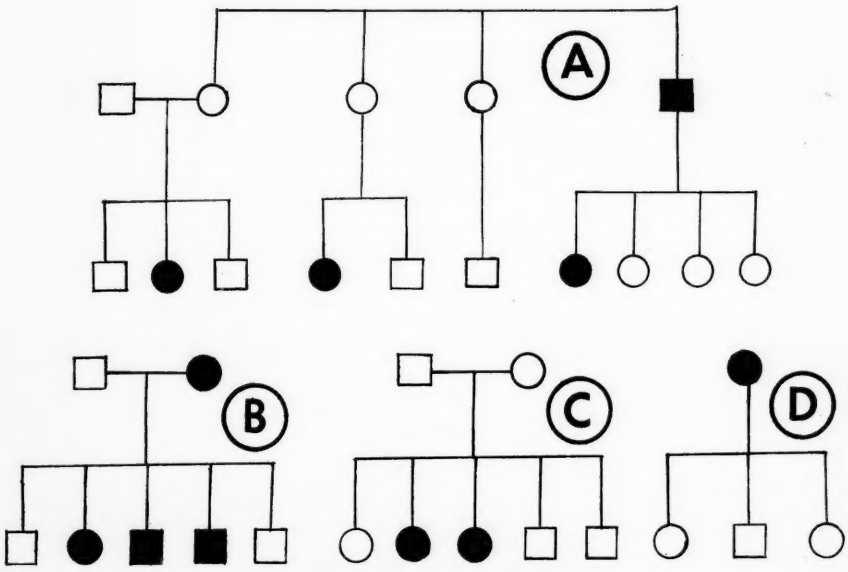

PEDIGREES OF TONGUE-FOLDING

Figure 5

In the Chinese population studied only about 30 tongue folders per thousand were noted. The inheritance suggests a recessive gene.

STURTEVANT[2] reported that the ability of rolling up the lateral edges of the tongue is conditioned at least in part by heredity. This ability, so far as the data show, may be due to a single dominant gene. Apart from the rolling up of the tongue edges, another character of tongue movement, not having been reported previously, has recently been noticed by the author among some of the Chinese population. A typical example of the character is shown by the accompanying picture of a girl who is a biology student in this university. She can make the distal part of her tongue turn up and fold itself back intimately upon its main body. Of course, this is performed without the aid of the teeth. Any one who does the trick can freely protrude and withdraw the tongue without any difficulty. Among the positives, some persons are able to acquire the character only after some practice. In one case a lady got the hang of it after two days of exercise. But the negatives could never do this regardless of practice. No intermediates have so far been noticed. This character, therefore,

can be discriminated into two opposite classes, namely, the positives and the negatives.

A preliminary survey of the distribution of the character reveals that the frequency of the positives as compared to the negatives is fairly low. In a total of about one thousand cases examined, only about thirty are positives. Since this ability can be determined only by direct observation, the pedigree data so far obtained do not cover more than two generations in each case. In one family (pedigree *B*), a positive mother and a negative father have three positive and two negative children. In another family (pedigree *A*) a negative mother has a positive daughter and two negative sons, but the mother's brother and her niece are also positives. These two instances demonstrate clearly the heritable nature of the trait.

In several cases, two negative parents can have one or more positive child or children. Pedigree *C* is an example of the kind. In one case (pedigree *D*), where the father's character is undetermined, a positive mother has three children, who are all negatives. Taking all these facts together, it appears that the ability of tongue upfolding is probably controlled by at least one recessive factor. The gene is not sex-linked, since the transmission of the gene as shown in these pedigrees does not reveal any sign of criss-cross inheritance.

In the seven families that give positive children from the two negative parents, the result of the calculation (see following table) in accord with the formula suggested by Hogben[1] shows the fit of the data to the assumption of a simple Mendelian ratio. The discrepancy between the expected and the observed numbers is 2.668, which is smaller than twice the value of the standard deviation (1.84).

TABLE I. Analysis of Families Showing Tongue Upfolding Character

Known size of fraternity	Family number	Upfolding members		Variance
		obs.	exp.	
2	1	1	1.43	0.122
3	1	1	1.297	0.263
4	2	3	2.926	0.840
5	2	2	3.278	1.184
7	1	1	2.020	0.970
Total	7	8	10.668	3.379

Population studies of this gene and of the dominant "rolling" gene in China are in progress. The relationship of these characters and the statistical study of the frequency distribution of these genes will be presented in a separate paper.

Literature Cited

1. Hogben, L. *Nature and Nurture.* George Allen & Unwin Ltd., London. 21939.
2. Sturtevant, A. H. *Proc. Nat. Acad. Sci.* 26:100-102. 1940.

✦

Double-Curl Tongue

Urbanowski and Wilson's brief report in the December 1947 Journal of Heredity on the frequency of tongue curling elicited a report of another kind of tongue-curler, different from the usual "tube-tongue" school and from the tongue-upfolders reported above. This gifted young man was able to twist the tip of his tongue into two curls instead of the more conventional tube. The accompanying sketch (Figure 6) shows a frontal view of the tip of the tongue curled according to this unusual manner, and protruded between the lips.

DOUBLE TONGUE CURL
Figure 6
Is this super-curling inherited?

No information is at hand as to whether other members of his family can twist their tongues into this odd shape. It is hoped that further information will be forthcoming.

Other readers of the Journal may encounter other lingual vagaries. Notes on any such cases, especially if they run in families, would be of interest. Non-pathological hereditary differences serve a very useful purpose in lectures and discussions on human heredity. And if such peculiarities are inherited in a simple Mendelian manner, they may have permanent value in linkage studies which eventually will give us a map of the human genes and chromosomes.—R. C.

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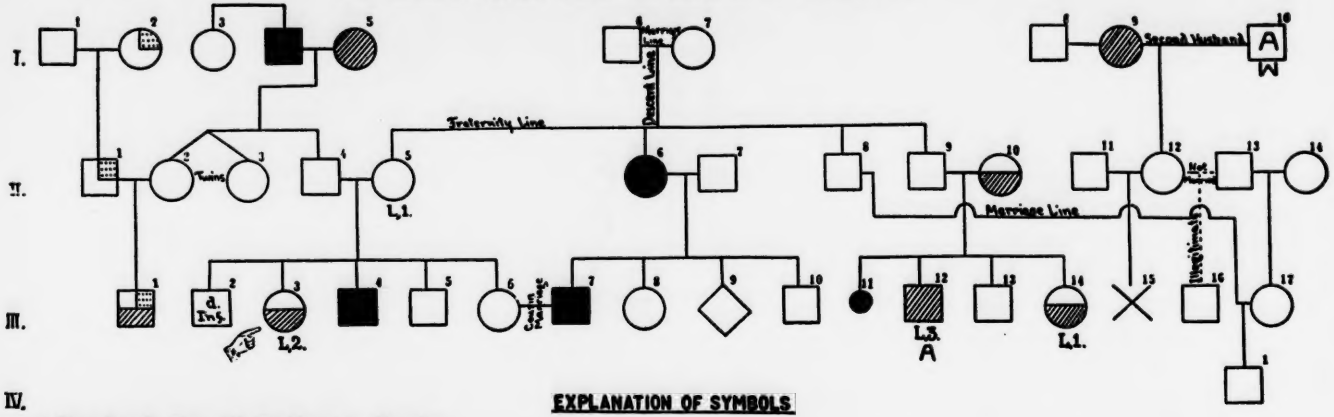
SAMPLE PEDIGREE CHART SHOWING THE MANNER OF CONSTRUCTION, AND THE USE OF STANDARD AND SPECIAL SYMBOLS.



EXPLANATION OF SYMBOLS

(a) Standard for all Pedigree Charts:

□=Male; ○=Female; ◇=Sex unknown; ●=Still-birth or Miscarriage; X=Children — number and sex unknown; ○○=Twins; d. Inf.=Died in Infancy; ☞ Points to the Propositus or central figure in the pedigree.

□—○=Parents.

□○□○○=Children.

Roman figures to the left indicate generations; Arabic figures locate individuals, (thus III. 7 is the young man in the third generation who married his cousin).

The following letters, placed in or around the individual's pedigree symbol, are standard for certain traits: A, alcoholic; B, blind; D, deaf; E, epileptic, F, feebleminded, I, insane, M, migraineous, N, normal in reference to traits under consideration; Ne, neurotic; P, paralytic; Sx, sexually immoral; S, syphilitic; T, tuberculous; W, wanderer.

(b) Especially devised or selected for this particular pedigree-chart:

■●=Successful leaders in politics. ◱◷=Extra thumb on right hand. L.1.=Highly successful author. L.3.=Little or no ability in literary efforts. ▨◍=Superior in vocal music. ▨◍=Medium attainment in vocal music.

To fit the particular family and traits (whether physical, mental or temperamental; good or bad) under consideration, invent special symbols, or select special letters (in addition to those standardized under (a) above) to be placed within or near the particular individual's pedigree symbol, to indicate particular traits and their degree of development.

Prepared by the International Commission of Eugenics. See *Journal of Heredity* for August 1926. Additional copies may be obtained from A.G.A.

Prepared by the International Commission of Illumination